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Comment on acp-2020-1174

Anonymous Referee #2

Referee comment on "The CO_2 integral emission by the megacity of St Petersburg as quantified from ground-based FTIR measurements combined with dispersion modelling" by Dmitry V. Ionov et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-1174-RC2, 2021

This work estimates the total carbon dioxide emissions from the city of St. Petersburg, Russia. In order to do this, a top-down approach is used, which uses total-column CO_2 measurements from two field campaigns, an a priori emissions map (from ODIAC), and an atmospheric transport model (HYSPLIT). The authors conclude that the observed CO_2 suggest that the true emissions of the city (~76 Mt/yr for 2019, ~68 Mt/yr for 2020) are more than double the bottom-up estimate (~30 Mt/yr).

Although the amount of data used is quite limited (11 days using 2 instruments in 2019, 6 days with one instrument in 2020), the authors hope to build on previous studies that have shown the utility of groups of EM27/SUN sensors to detect small enhancements in trace gas column concentrations associated with urban emissions. Their ultimate goal is to use the little data they have to determine the emission rate of the entire city. Anyone who has attempted to infer urban emissions from scarce atmospheric observations of this kind will recognize the difficulty of this task, as there are numerous sources of noise and uncertainty that are hard to account for. This paper falls short, however, on presenting a convincing method for retrieving an urban emission rate using this data. Most strikingly, the entire manuscript lacks detailed equations describing exactly how the FTIR data, transport model, and ODIAC data, and combined. I would expect a paper that presents a data-model fusion product like this to not only have extensive equations and tables, but also a supplement with additional tables and figures, but there appears to not be any supplemental information provided. This manuscript heavily cites a previous work by one of the co-authors (Makarova et al. 2020) but readers should not have to dig through a cited paper to understand the basic methods being used in this work to motivate its main result. Even after thoroughly reading the Makarova paper, it is still difficult to understand exactly how the data were processed and used in this current work. Even the EM27 data itself is not presented clearly in this manuscript. It would be useful to see a couple of daily time series plots of the XCO_2 data from both sensors so the reader can see not only the difference between them, but the (likely) large hourly variations typically seen by urban EM27 instruments.

It is not entirely clear how background XCO_2 concentrations are determined. For the 2019 campaign, when 2 FTIRs were used, it appears that the sensors were placed such that one was inside the "urban plume" and that one was placed outside of this plume. The sensor outside the plume is then assumed to be the background, but there is nothing presented in this manuscript that builds confidence that this a reasonable assumption. Is the background site even upwind of the city? What is the uncertainty associated with this decision? Are there emission sources upwind of this background site? For the 2020 data, the background determination is even worse, as only one instrument was available, so the sensor was moved during the course of the day in an attempt to capture a useful background value. Unfortunately, total-column CO_2 concentrations can vary greatly over the course of a day, and it is not uncommon for background variations to be on the order of a urban emissions signal, making this assumption unadvisable. It appears that there was no attempt to quantify the uncertainties associated with these assumptions about the background- again, no equations are given.

The implementation of the transport model is also questionable. The authors state that they are using the HYSPLIT dispersion model, but nowhere in the figures or texts does it appear that any dispersion is actually being simulated. It is unclear, but it looks like HYSPLIT was configured to run backwards in time to compute single particle trajectories, with no stochastic (dispersion) component. It is then stated that "The width of the air paths was assumed to be 10km" [Line 262], which I assume means that plume of influence on each observation is simply modelled as a straight line 10km wide. This type of modelling would suggest that the column observed is equally sensitive to emissions 500 meters upwind as it is to emissions 15 km upwind, which is incorrect. It is then unclear how surface emissions are "integrated" into the column based on these trajectories. Also, how is vertical transport dealt with? Are particles that rise to the top of the boundary layer treated the same as those that travel closer to the surface?

The current version of HYSPLIT is able to run in a mode that actually simulates dispersion and surface influence on observations, using the Stochastic Time-Inverted Lagrangian Transport (STILT) model. The HYPSLIT-STILT model produces a influence function (footprint) with the correct units (ppm / umol/m2s) to relate surface emissions to atmospheric observations, and have been used many times in studies with similar goals as this one. I would strongly suggest using this, or a similar model, to reprocess these results.

It is unclear (due to the lack of math presented) how the observations, transport model, and prior inventory are combined to produce the resulting emissions scaling factors and uncertainties. Did the fitting process take into consideration different uncertainties in the model and observations? It is mentioned that “The error assessment for the scaling factor should be discussed is some detail” [Line 231], however this is followed by only a few sentences which present an error analysis that does not account for any large sources of error, such as errors in the transport do to wind speed and direction uncertainty, or errors due to uncertainties in the background estimate or spatial distribution of emissions.

It is my opinion that the work as is does not present a robust, reproducible, or innovative analysis that adds scientific value to the dataset. Although it may be possible to infer information about the CO_2-emissions of the city of St. Petersburg from these observations, a much more thorough analysis would be needed, and would require significant effort from the authors.